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SCIENTIFIC REVIEW OF THE ONTARIO CLEAN AIR PROGRAM DISPERSION MODELS

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ABSTRACT

An external scientific review of the Ontario Ministry of the Environment (OME) Clean Air Program (CAP) modelling package was undertaken. This report provides a synopsis and brief discussion of the reviewers' comments for inclusion in Ontario Hydro's response to the OME's invitation for industry comment. The scientific reviewers considered the CAP modelling to represent a good compromise between the need for simplicity and an adequate representation of the physics of dispersion in the planetary boundary layer. Specific suggestions to improve the modelling package's ability to represent Ontario Hydro's unique source and dispersion conditions were made. It is recommended that Ontario Hydro investigate implementing these improvements, and that a cooperative program between the OME and industry be undertaken to test and improve the CAP modelling system.

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1.0 INTRODUCTION

An external scientific committee was formed to peer review the Ontario Ministry of the Environment proposed Clean Air Program air quality modelling package. The role of the scientific review was two fold, one, to provide information for Ontario Hydro's reply to the OME on the proposed CAP modelling protocol and, secondly, to identify research initiatives to improve the ability of the CAP models to reflect source and dispersion condition unique to Ontario Hydro. This summary addresses the former aspect by offering a synthesis and discussion of the reviewers' comments.

Four scientists prominent in the field of air quality modelling were selected. The reviewers were: Professor John Wyngaard, University of Pennsylvania (formerly with NCAR); Dr. Steven Hanna, Sigma Research Corp.; Dr. Jeff Weil, NCAR; and Dr A.P. Van Ulden with KNMI Netherlands.

This report presents a discussion of the scientific acceptability of the CAP modelling package in Section 2. Section 3 summarizes the reviewers' replies to specific questions posed by Ontario Hydro, and Section 4 offers concluding remarks. The reviews submitted are found in the appendices.

2.0 SCIENTIFIC ACCEPTABILITY

All reviewers commended the OME for their effort to improve upon traditional regulatory modelling approaches, based on the use of Gaussian plume/Pasquill-Gifford-Turner dispersion assumptions, used in most other countries, by presenting a group of models with a more comprehensive description of the physics of the planetary boundary layer. All reviewers suggested that the OME package needs to be revised to reflect advances published over the past five years. Numerous references were suggested by the reviewers.

Wyngaard comments that better models do not always give clearly better results. This is attributable to the inherent uncertainty in observations. He goes on to say that only direct testing against observations can reliably reveal where an air-quality model needs improvement and that this testing-improvement cycle is an essential part of the practice of air-quality modelling. The other reviewers also emphasized the importance of model validation.

The main scientific limitations of the CAP modelling package, as identified by the peer review committee, can be summarized into five areas.

2.1 The Stability Classification by Heat Flux Alone (ie, proposed $-5\ W/m^2 < H_o < 5\ W/m^2$)

The surface heat flux, H_o, is a key parameter in the OME modelling system, especially in the determination of dispersion in the convective boundary layer, CBL. The reviewers suggested that the OME approach of using H_o alone to characterize the turbulence may be inadequate. In the CBL case Weil suggests that the turbulence be characterized by $-z_i/L$ or alternatively by w_*/U or w_*/U_*. In the stable boundary layer, SBL, he suggests the use of h/L where h is the depth of the

difference, in conjunction with an energy budget measurement, should be used to estimate H_s. These comments underline the importance of collecting site specific input data to improve the performance of the models.

2.2 Dispersion in Building Wakes

It was recognized by the reviewers that the building wake effect is a complex situation to be dealt with by a modelling system and that it may be necessary to combine analytical and physical modelling approaches to these problems. The main limitations of the proposed modelling approach are: the neglect of source momentum and buoyancy in the concentration estimates; the well-mixed assumption may be inappropriate for building wakes; and the approach for computing the concentration beyond the cavity region is overly conservative in cases where the effective release height is larger than the building height. Specific recommendations are made by Weil to improve the OME proposed building wake model. Hanna suggests the USEPA or USNRC models as alternatives, and Wyngaard suggests that direct measurements at a real site are needed. Wyngaard refers to recent measurements which showed high concentrations occurring at the outer boundaries of building wakes.

Ontario Hydro's facilities are unique in size, configuration and siting. Characterization of building wake dispersion will require a program coupling field measurement, with physical and analytical modelling.

2.3 Dispersion in the Lakeshore Environment

Comments on the OME lakeshore model are of particular interest to Ontario Hydro given that most of our facilities are located in lakeshore environments and that the dispersion scenario presented by the lakeshore model can result in high glc's. The reviewers expressed concern over the limited scope of the validation of the lakeshore model. They submit that the model is particularly sensitive to the depth of the growing internal boundary layer. The reviewers suggest that this depth cannot be reliably predicted without direct local measurements of the internal boundary layer. Specific suggestions for improving parameterizations within the lakeshore model (plume penetration, TIBL shape, TIBL temperature profile, lateral spread module) are made by Weil. Comparison with the USEPA shoreline dispersion model is suggested by Hanna.

2.4 Highly Buoyant Plumes in the Convective Boundary Layer

Another limitation of the OME modelling package that is of particular interest to Ontario Hydro is the dispersion of highly buoyant elevated releases in the CBL. The buoyancy flux, F, of our thermal power plant plumes is typically 1000 to 1500 m^4/s^3 and, from the BHWP flare stack, it can be as high as 5000 m^4/s^3. This translates into dimensionless buoyancy fluxes, F_* (ie, $F_* = F/w_*^3 U z_i$), of much greater than 0.1. Plumes in these cases have been shown in laboratory experiments (Willis and Deardorf 1983, 1987) and by field observations (Hanna and Paine 1989) to remain near the top of the CBL and disperse slowly to ground resulting in lower glc's than would be predicted by a CBL model which does not consider this plume-lofting phenomenon.

Predictions a factor of two or three greater than observations for these cases have been reported by Weil and Corio (1988). Specific suggestions on how to model these dispersion cases have been submitted by Weil.

2.5 Empiricism and Parameterization

The OME modelling package has been designed to require a minimum of direct measurements of parameters. It thus relies heavily on inferential methods using routine meteorological data and empiricism to derive the required parameters. The reviewers have identified many areas for improvement in the parameterization. Hanna suggests there are many unjustified empirical constants requiring references and figures to demonstrate their origins.

An example is the empirical relationship proposed by OME to calculate the Monin-Obukov length L in stable atmospheres (ie, $L = 1100\ u_*^2$). This relationship is based on data from a bare soil agricultural site in flat terrain. The theoretical basis for such a relationship has not been explained by the author, Venkatram (1980). Another example is the inversion jump of 10°C proposed for the plume penetration model. Weil suggests values of 1-2°C through the CBL. Weil suggests an alternative penetration model based on Weil (1988).

The reviewers indicated several areas where direct measurement of a parameter or additional measurements would be required to improve parameterization. These were measurement of boundary layer depth, the standard deviations of horizontal and vertical wind fluctuations at 10 m, temperature difference between 2 and 10 m in unobstructed surface layers, net radiation, and, in the case of elevated releases, wind data near the source.

3.0 ANSWERS TO QUESTIONS POSED BY ONTARIO HYDRO

Five specific questions which had been raised in-house were provided to the reviewers for their comment. Below is a summary of the replies to four of these specific questions. The fifth, concerning the building wake model, has been addressed above.

3.1 Have the Models Been Adequately Tested?

Three of the four reviewers felt the models had not been adequately tested. The fourth reviewer did not comment. Specifically, it was suggested that Venkatram's (1980) model for the convective boundary layer and MISRA's (1980) lakeshore model had only been tested with limited data; further testing should be performed before the models are used in the context intended in CAP. As well, it was suggested that the proposed stability classification method required further testing. Wyngaard suggests that ongoing testing is necessary for proper use of such a modelling system. Weil calls for extensive performance testing of the package, preferably against a year of observed hourly maximum glc's.

3.2 How Should the Results of These Models be Interpreted?

The reviewers that responded to this question agree that the models proposed by the OME provide an estimate of the expected value or ensemble average of the concentration at a given location for a given emission and meteorological condition. The scatter in an observed concentration about a predicted time average has been shown by experience for models of this type to be about a factor of two. These models will not predict peak concentrations or the probability distribution at a given receptor.

3.3 Are Data Requirements Realistic?

The implications of assembling the one year's worth of input meteorological data required to run the proposed multi-source model, were not addressed by the reviewers. Data requirements, with respect to quantifying model parameters, were in general thought to be minimal and, in fact, more measurements were thought to be needed (see above).

3.4 Are Assumptions Too Conservative?

Assumptions identified by the reviewers as being too conservative include: the inversion jump in the plume penetration model; the use of surface winds for elevated plumes; the neglect of the source momentum and buoyancy flux in the building wake modelling; the assumption of $z_o = 0$ for an elevated release in the building wake model; no penetration assumption in the lakeshore model; the dispersion in calm conditions module; the treatment of elevated releases into a stable boundary layer when release heights are above the stable layer depth; and the TIBL depth parameterizations in the lakeshore module.

4.0 CONCLUDING REMARKS

An external peer review of the OME proposed CAP models has been completed. In general, the reviewers complimented the OME for assembling a modelling package that reflects much of the present view of the physics of the planetary boundary layer. That being said, the reviewers went on to make extensive comments regarding the modelling package.

The following conclusions and recommendation are drawn from the scientific review:

- the modelling package needs to be brought up-to-date by implementing recent advances in boundary layer meteorology published over the past five years.

- Areas for improvement to the CAP modelling package suggested by the reviewers, that are particular interest to Ontario Hydro are: the lakeshore model; the building wake model; the stability classification; data requirements; and buoyant plume rise in convective boundary layers. Ontario Hydro should consider implementing improvements to the models in these areas.

- It is recommended that a cooperative program between the OME and industry be undertaken to test and improve the modelling system.

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APPENDIX A

John C. Wyngaard

Review of the MOE Clean Air Program Modeling Package

submitted to

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by

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1. General comments

1. Overview

The authors of the MOE modeling system have assimilated many of the recent research advances in boundary-layer meteorology into a package that seems manageable to describe and to use. In my view, they have shown enlightened judgment in striking a delicate balance between the need for more comprehensive physics and the pressure for simplicity.

As summarized in Tables 1.1a and 1.1b, the MOE system uses modeling methods that vary with the application. The simple situations of Table 1.1a, being the best understood, are treated through relatively noncontroversial techniques. The special situations in Table 1.1b—lakeshore, complex terrain, and building wakes—are more difficult and their modeling is more controversial. In my view, it has been very difficult to develop general modeling guidelines from the community's experiences with measurements in complex terrain and building wakes. I think the recommendation of physical modeling in the more difficult cases within these categories is realistic. The other special situation, the lakeshore setting, is treated through an analytical approach that the literature shows can be successful.

Assessing the likely performance of this model system is not easy. The behavior of any complicated system, hardware or software, with state-of-the-art components is best determined through use. The care with which these components are designed, built, and integrated into a system can have a dominant influence on the performance. For anyone but a deep specialist in such systems, performance predictions are hazardous. I will not attempt one.

2. A comment on submodels

Over the past decade there have been efforts within the US to stimulate improved air-quality modeling. Some of these stem from a special committee of the American Meteorological Society. Established in about 1980 under the name "Committee for the Cooperative Agreement with the EPA," it has sponsored a number of workshops on leading issues in air-quality modeling. One held in January, 1984, under the chairmanship of J. C. Weil, reviewed current understanding of the planetary boundary layer and its use in diffusion modeling, and presented improved model components. Seven papers from that workshop were published in the November, 1985 issue of the *Journal of Climate and Applied Meteorology*. These provide an overview of the field and give details of recent advances in parameterization and modeling.

The authors state that they selected model components based on a review of the recent scientific literature, but none of these papers is cited. I presume this system was built after 1985, so this oversight is worrisome. I would like to be sure that improved versions of the submodels used within the MOE system have not been overlooked.

3. The use of air-quality model systems

In my view, there are two striking aspects of the comminity's experience with air-quality models over the past 20 years. First, it was found that better models do not always give clearly better results. Models having vastly improved physics can still have discouraging "scatter plots" of predictions versus observations. The reason is believed to center on "inherent uncertainty"'—the inevitable difference between the most likely or expected outcome over an ensemble of realizations of an experiment, and the behavior in an individual realization. The scatter between individual observations and the predictions of even a perfect model disappears only asymptotically as the averaging time becomes large. Unfortunately, in air-quality applications much longer averaging times than are used in practice—or are even possible in some applications—are typically required to give small scatter.

The second aspect is related to a point I made above—that only direct testing against observations can reliably reveal where an air-quality model needs improvement. Even a system such as this, put together by active researchers from up-to-date submodels of PBL processes and behavior, will undoubtedly have deficiencies. As testing reveals these weak points—through failures in certain types of conditions, for example—they can be remedied. Experience has shown this testing–improvement cycle to be an essential part of the practice of air-quality modeling.

The implications here are twofold. First, a user of the MOE model system should be prepared for large deviations from the model predictions in individual runs, if only because of inherent uncertainty. Doing better calls for a different type of model—e.g., a probabilistic one that predicts not only the most likely outcome (the mean), but also the expected range of values (the standard deviation, or scatter) about this mean. Such models are not yet ready for use, although they are attracting interest (Weil, 1985). Second, this model system should ideally be subjected to long-term testing against observations, either existing or new. I believe that only in this way can it receive the refinements and tuning that will likely be needed for it to reach its potential.

4. The role of observations

The MOE model system is based on an up-to-date view of the boundary layer and dispersion within it. It uses the surface fluxes, boundary-layer depth, the convective velocity scale, the friction velocity, and the Monin-Obukhov length in its submodules. Few if any of these were used in the models of a generation ago. This represents real progress.

It is also designed to require minimal input from direct measurements. Instead, it derives most of its advanced parameters from routine meteorological measurements and parameterizations of boundary-layer physics. For example, the surface heat flux is obtained through a parameterized surface energy budget based mainly on the net radiation balance. That, in turn, is inferred largely from land type, season, and time of day. As another example, the boundary-layer depth in neutral and stable conditions is inferred from formulas developed from research results for equilibrium boundary layers. Only in the convective case are measurements used—there it is taken as the inversion height as judged from the temperature profile.

This "minimalist" policy on measurements seems not to be universally accepted within the community. It stands in contrast with the stated consensus of a 1980 workshop on instrumentation requirements for diffusion from point sources. As summarized by Hoffnagle et al. (1981), that group considered direct measurements of the following parameters, among others, to be essential:

> standard deviations of horizontal and vertical wind fluctuations at 10 m;
> temperature difference between 2 and 10 m in unobstructed surface layers;
> boundary-layer depth.

The first of these gives a direct measure of the dispersion properties of the atmosphere; the second yields the heat flux, a key similarity parameter; and the third gives the depth of the fully turbulent layer through which pollutants are dispersed. Since each of these is of zero-order significance to dispersion, one can easily understand why they might be regarded as essential inputs. None is required by the MOE system.

I judge that the input data requirements of the MOE system are close to one extreme; it is difficult to imagine less stringent and still realistic requirements. Only routine meteorological measurements are called for, which minimizes the expense of data gathering and analysis. At the other extreme would be a requirement for direct measurements of all relevant boundary-layer parameters. This might not be out of the question today, given the wealth of indirect techniques for use with in-situ sensors and

the wider availability of remote sensors. Clearly, it would require more complicated and expensive equipment, more highly skilled people to use it, and more care in using the model system.

The trade-offs here are even more complex. Any air-quality modeling system needs representative values of its meteorological input parameters. It is difficult to quantify this. Qualitatively, it means that in typical applications, where fetches are not uniform and conditions are generally not as simple as we idealize them in models, we need input parameter values that are more than locally representative; we need values that are representative on the scale of the dispersion problem of interest. It is not clear that one can achieve the desired (if ill-defined) representativeness through direct, local measurements of the parameters. There is reason to believe that indirect measurements can be more representative, in this sense, than direct ones.

In summary, I believe that the question of input data requirements for an air-quality modeling system is, in itself, a complicated and unresolved issue. I am not aware that it has yet been studied in its full complexity, or that there is a community consensus on it. Nonetheless, I do feel that some additional call for direct measurements—specific examples of which I will list later—could enhance the performance of the MOE model system.

2. Specific comments on system

1. The surface heat flux

The surface heat flux H_0 plays a dominant role in the MOE modeling system. It is the sole indicator of whether the boundary layer is stable, neutral, or unstable, for example. Within the convective category it strongly influences the dispersion parameters.

The system derives the heat flux from a model of the surface energy balance—specifically, (2.1.1). In general that involves H_0, the latent heat flux, net radiation, and the soil heat flux. Van Ulden and Holtslag (1985) discuss this balance in the context of air-quality model needs. The principal difficulty in this approach is determining the latent heat flux contribution. Weil (1985) writes "In general, the characterization of surface moisture is the most significant problem [in predicting heat flux in this way]."

To my eye, it would be better if there were some redundancy in H_0 estimates. For example, two-point (vertically separated) mean temperature differences are a good, inexpensive, traditional method of inferring H_0. Use of such techniques, possibly in conjunction with the energy budget, would seem to yield more robust estimates than (2.2.1) alone can.

2. Boundary-layer depth

In the neutral case H is defined through (2.2.8), which according to recent research is the depth of the classical neutral Ekman layer. I doubt whether there are many cases in the real atmosphere where the physics behind (2.2.8) is actually operative. One reason is that it takes a long time—several $1/f$ periods, where f is the Coriolis parameter—for a neutral Ekman layer depth to equilibrate at this value. The atmosphere is seldom steady that long.

The other problem with (2.2.8) in practice is its neglect of any elevated inversions. Quoting Van Ulden and Holtslag (1985), "In practice...often elevated inversion layers exist even when a major part of the ABL can be considered neutral. In that case the ABL depth is limited by the height of the elevated inversion. When observations...indicate the presence of an inversion at a height less than that given by [an expression equivalent to (2.2.8)], the inversion height should be taken as ABL depth...."

If (2.2.8) works, I believe it is because it tends to give (for the wrong reasons) values in about the right range. It is not apt to give values that are substantially too small. It could give serious overestimates if low inversions are present, but these cases could be easily detected with acoustic sounding. This is another situation where direct measurements could be very useful.

3. Stable boundary-layer structure

The parameterization (2.2.10) was developed empirically and without a theoretical explanation by Venkatram from the 1973 Minnesota data. Those data are from a bare-soil agricultural site in flat terrain. Van Ulden and Holtslag (1985) developed a model that allows for more variability.

Parameterization (2.2.11) is used for boundary-layer depth H. This is an equilibrium result based on model studies that are substantiated by only a few, special observations. Under stable conditions H is typically only a few hundred meters and can be measured directly with an acoustic sounder, for example. This strikes me as a better way of determining H.

The stable Ekman layer can have considerable ($\sim$ 45°) turning of the mean wind with height. It can also have large wind shear—particularly if there is a nocturnal jet at its top. There the wind speed can be considerably larger than geostrophic. In longer-range applications this can have substantial impact on diffusion patterns.

4. Diffusion parameterizations

Chapter 3 of Appendix 8-2 deals with the detailed parameterizations used to account for the various influences that determine the effective release height of the sources. Chapter 4 deals with the methods used to calculate the resulting concentration downwind.

I cannot comment on the relative merits of the competing parameterizations of these processes from which the authors chose. Again, however, I do feel that they might have inadvertently raised questions about the fullness of their selection process by limiting their references to the relatively early literature. Good, up-to-date reviews have been done by Briggs (1988), Venkatram (1988), and Weil (1988). I would like to be sure the authors have not missed any new developments that appeared in these later references.

5. Building-wake effects

This is perhaps the most complicated situation dealt with in this modeling system. The underlying science is of less direct use here, and it is necessary to rely more heavily on empiricism and engineering modeling. This conceivably could change as we learn more about diffusion in wakes, of course, but I believe it does reflect today's state of affairs in the air-quality modeling field.

In my view, the authors have assimilated well the relevant background research in this area and have formulated a workable approach. In the case of individual buildings, they differentiate between structures of "simple" geometry and building clusters. The authors warn on p. 2-47 that for buildings whose geometry does not qualify as simple, "...the concentrations of pollutants computed...may be subject to relatively large errors." Building clusters are defined and parameterized separately. Again, there is mention of the acceptability of physical modeling, to be done at the discretion of the proponent.

This component does not have the solid "feel" of the others in this model system—nor could it. By its nature, this is an area of much greater uncertainty.

Two points come to mind. First, I understand from Ray Hosker (NOAA ATDD, Oak Ridge; personal communication) that recent fast-response video or film measurements in building wakes indicate that the intermittent vortex separation from the structure can bring very high concentration (i.e., undiluted) effluents to the center portions of the wake. The concentration time series would show these as occasional, high-amplitude bursts. A time-averaged concentration might not reveal anything out of the ordinary, but should the material be toxic, these bursts could be quite important.

This suggests that the occasional "well mixed" assumptions this system uses in building wakes (Appendix A) should be treated with caution.

The second point is that given the relatively rougher state of this area, direct measurements at a real site would be particularly welcome (but perhaps also particularly difficult to make and interpret).

6. Modeling in a lakeshore setting

Appendix C of Appendix 8-2 details a modeling approach for the special situation near lakeshores when the flow is from colder water to warmer land. This would appear to be of importance to Ontario Hydro, all of whose thermal generating facilities are located on the shoreline of a large body of water.

In this situation the convective boundary layer near the surface is shallow at its origin near the water-land interface, but grows in depth downwind. The elevated plume that is discharged into the stable air aloft is entrained into this internal boundary layer at some point downwind. The MOE model system thus uses two submodels here: one for the downwind growth of the internal boundary layer, and one for the entrainment and downward dispersion into this layer. As discussed by Venkatram (1988), this general approach was found by Misra and Onlock to be successful in predicting SO_2 concentrations at Nanticoke; most of the predictions for the two days shown were within a factor of two of the hourly averaged, ground-level measurements.

I do have one concern. Venkatram (1988) as well as the authors of the MOE system stress that this shoreline fumigation model is sensitive to the depth of the growing internal boundary layer. This depth is quite difficult to predict reliably, particularly without very detailed measurements of the local environment. Thus, I suggest that direct, local measurements of the internal boundary layer depth be made. The authors indicate how this can be done.

3. Response to specific questions

1. Have the models been adequately tested?

No. As I pointed out in section 1, I believe that testing is part of the proper use of such a model system.

2. How should the results of these models be interpreted?

I believe a result of an air-quality model of this type should be interpreted as an estimate of the *expected value* of the concentration in a given place for the

specified emissions and meteorological conditions. (I use *expected value* in the sense of probability theory, where it is another term for ensemble average.) As such, it can differ substantially (easily by a factor of two, experience shows) from an individual measurement of time-averaged concentration in a given realization.

3. Are data requirements realistic?

In my view, they are minimal. As I explained, I think it would be good to increase them somewhat.

4. Are assumptions too conservative?

If by *conservative* is meant moderate, prudent, safe, (which in this context means yielding overestimates of concentration) then I see no real way to judge at this point. Experience will tell. I do not see a consistent attempt here to give overestimates.

If by *conservative* is meant resisting innovation or change, then I say no. The MOE modeling system is clearly innovative.

5. Is the proposed approach to calculating building wake concentration too conservative?

Presuming the first meaning above, I do not believe so. In fact, I pointed out that it seems to ignore the very high amplitude but short duration effluent bursts that occur in building wakes.

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APPENDIX B

J.C. Weil

REVIEW OF THE CAP MODELING PACKAGE

by

J. C. Weil

January 1991

I. Introduction

The CAP modeling package is an assembly of submodels to predict dispersion and ground-level concentrations (GLCs) from a variety of stationary sources in the planetary boundary layer (PBL). On the whole, the submodels incorporate state-of-the-art approaches for estimating PBL variables, source buoyancy effects, dispersion, GLCs, etc. In general, I think that they are a scientific improvement over the traditional Pasquill-Gifford-Turner (PGT) dispersion and stability classification approach routinely used in most countries (including the USA). However, the CAP model does contain some scientific limitations with potential practical consequences, i.e., to the dispersion and GLC estimates. The most notable ones are: 1) the definition of stability by heat flux (H_0) alone, 2) the treatment of highly buoyant plume dispersion ($F_* > 0.1$) in the CBL, 3) the modeling of dispersion in calms using an assumed minimum wind speed, 4) the treatment of dispersion in building wakes, and 5) the modeling of certain aspects of the lakeshore problem.

In the following, I: 1) respond to the technical questions raised within Ontario Hydro, 2) discuss the main scientific limitations of the CAP model, 3) give itemized comments on the model, and 4) provide some recommendations on implementing CAP.

II. Questions Raised Within Ontario Hydro

1. <u>Have the models been adequately tested?</u> In general, they have not. The Venkatram (1980) model for the convective boundary layer (CBL) and Misra's (1980) model for shoreline fumigation have been tested in limited intensive plume experiments where the meteorology is reasonably well known. However, they have not to my knowledge been tested for routine predictions at specific sites over a one-year period; i.e., to see how well they predict the maximum 1-hr GLCs over a year which is the typical period for standards assessment.

From personal experience with an updated Gaussian plume model (PPSP) (Weil and Brower, 1984; Weil and Corio, 1985, 1988; Tikvart and Cox, 1984), I know that a model tested under well-defined but limited conditions does not guarantee its performance with routine (hr-by-hr) monitoring data over a year (see also Sections 3.1, 4.2 of Weil et al., 1990). The problem is that certain meteorological scenarios do not arise in limited validation experiments of the sort used by Weil and Brower (1984), Venkatram (1980), and Misra and Onlock (1982); therefore, model predictions for such scenarios do not receive testing. In the case of the PPSP model (Weil and Brower, 1984), the problem was the treatment of highly buoyant plumes that "loft" near the top of the CBL (high F_*). The original model typically overestimated the maximum hourly-averaged GLCs over a one-year period by factors of 2 to 3 (see Tikvart and Cox, 1984). A modified model performed much better (Weil and Corio, 1988).

The stability classification method in the CAP model (page 2-10) is insufficient and has not been tested as far as I can tell. It relies on heat flux as the key variable; however, stability in the PBL also depends on mechanical or shear-generated turbulence which is characterized by the friction velocity, u_* (see Section III.1). In addition, two other models have not been tested: 1) the building wake model, and 2) the dispersion model for elevated releases during neutral and stable conditions.

2. How should the results of these models be interpreted? The answer varies with the model. For the Venkatram (1980) and Misra (1980) models, the predictions are of ensemble-mean 1-hr GLCs for a given set of meteorological conditions; there is scatter in the observed concentrations (C_O) about the predicted (C_P) ensemble-mean value. Typically, the geometric standard deviation (GSD) of the ratio C_P/C_O is 1.8 to 2 for these models (e.g., see Venkatram, 1980; Weil and Brower, 1984). I do not believe that these or any of the other submodels in the CAP package predict the correct probability distribution of C_O at a given receptor. To do so requires the incorporation of: 1) the stochastic variability in concentration about the 1-hr ensemble-mean value (such variability accounts for much of the above GSD), and 2) the uncertainty/variability in the model inputs (see Lewellen and Sykes, 1989, and Weil et al., 1990, for a discussion of this). These two factors are not included in the CAP model nor any other regulatory models with which I am familiar.

The building downwash model may be conservative for the ensemble-mean concentration (i.e., overestimate it; see Section III.4), but there is insufficient data at hand to say how much the overestimation is. However, if one is interested in predicting peak GLCs for averaging times of a few minutes, the models may not at all be conservative. For example, such predictions might be necessary in the case of toxics from an incinerator or of radioactive material released near a building. Peak concentrations for an averaging time of a minute or less could be of

the order of the source concentration as a small plume is brought to ground by the large eddies in the building wake. Further review of existing wind tunnel results is necessary to see if they give probability distributions of concentrations at fixed receptors in building wakes--for the estimation of peak short-term concentrations. If they do not, new experiments are needed for this problem.

I believe that the models should be designed to predict the correct ensemble-mean concentration for a specified averging time as in the case of the Venkatram (1980) and Misra (1980) models (i.e., for 1/2 to 1-hr averages); conservatism of an arbitrary nature should not be built into the model calculations since it may be more than adequate for some problems and inadequate for others. Fluctuations about the mean should be treated statistically using a probability distribution (see Weil et al., 1990, for discussion).

3. Are data requirements realistic? Yes, in that they are not excessive. However, there are a number of problems for which better data are desireable.

a) For elevated stack releases, routine wind data at or nearer the source height (than the 10 m level) are necessary especially under stable conditions. To get better estimates of H_0, insolation or net radiation measurements should be made especially for the CBL and a reliable method is needed for determining the Bowen ratio (B), B = H_0/latent heat flux. The latter ratio (B) is necessary in the surface energy budget to determine α in the relationship, $H_0 = \alpha R$, where R is the insolation. In CAP, α = 0.35 is assumed for all surfaces except snow (α = 0), but α is sensitive to B (i.e., the latent heat flux) for vegetated surfaces. Uncertainty in the surface moisture conditions is probably the greatest problem in estimating H_0 by the energy budget method (Weil, 1985).

b) Better wind information than from a single 10 m level sensor would also be necessary in complex terrain.

c) For the neutral (NBL) and stable (SBL) boundary layers, acoustic sounder measurements of the boundary layer height would be preferred to the use of Eq. (2.2.11).

d) Climatological data on the over water stratification ($\partial\theta_w/\partial z$) and $z_i(x)$ as a function of time of day and season are needed for the lake shore problem because of the sensitivity of the GLCs to z_i. Total reliance should not be placed on the z_i model since it has only been tested in limited intensive experiments, i.e., not over a long time record to capture a wide variety of conditions and possibly some under which the model would be in serious error.

4. Are the assumptions too conservative? Some are and among the more notable ones are the following.

a) The inversion jump (10°C) assumed in the plume penetration model (Eq. 3.4.3) seems much too high for the typical temperature jump above the CBL; a value nearer to 1°C to 2°C would seem appropriate. In addition, it may be better to

use a penetration model based on a thick elevated stable layer (Weil, 1988a) since the lapse rate in the elevated layer is that from an early morning sounding and may be easier to specify than a jump ($\Delta\Theta$).

b) The neglect of the source momentum and buoyancy fluxes in the building wake problem is certainly conservative, but there is no accepted model at present for estimating the effect of such fluxes on dispersion and GLCs. Weil and Furth (1977) proposed a very rough model for estimating GLCs for roof-top emissions but it was not tested with field or laboratory data.

c) The assumption of $z_e = 0$ (Eq. 3.1.1) for an elevated release in a building wake is conservative for estimating ensemble-mean GLCs in the wake. Huber and Snyder (1976) showed that for elevated nonbuoyant releases on or near buildings, the main effect of the wake turbulence was to enhance the vertical dispersion with the "effective source height" (z_e) being essentially the same as the source height. The peak GLCS decreased with increasing release height. (I think that the Huber and Snyder results on wake dispersion-- σ_y and σ_z --should be used in CAP rather than some of the assumptions in Appendix A.)

d) For a trapped plume in the lakeshore problem, it is assumed that z_i should be evaluated at the source location and that no penetration be considered. These assumptions, especially the second, may be quite conservative. Although the portion of a plume that penetrates the inversion will later be reentrained, the entrainment should not occur instantaneously as implied by no penetration. A delayed reentrainment will lead to increased dispersion and lower GLCs. Hence, I think that the trapped plume problem in the shoreline environment must be reassessed. In addition, for penetration based on the thick layer model (Weil, 1988a), the $\partial\Theta/\partial z$ above H would be the overwater value,

III. Main Scientific Limitations of the CAP Model

1. Stability classification according to $-5\ W/m^2 < H_0 < 5\ W/m^2$

The characterization of turbulence in the CBL should be determined by the surface heat flux (H_0), surface friction velocity (u_*), and z_i. Heat flux or convection effects extend to z_i, with the turbulence characterized by w_*, whereas friction effects (i.e., mechanical turbulence) are confined mostly to $-z < L$ and are characterized by u_*. The stability parameter measuring the relative importance of convective and mechanical turbulence throughout the CBL is $-z_i/L$, with useful alternatives being w_*/u_* or $w_*/\bar{U}$, where U is the mean wind speed in the CBL (Deardorff, 1972; Weil, 1985,1988b). For example, Weil and Brower (1984) used U/w_* with an assumed resistance coefficient, $u_*/U = 0.05$. Strongly convective

conditions with a nearly universal turbulence structure (for $z > 0.1z_i$) exist for $-z_i/L > 10$; more neutral like conditions exist for $-z_i/L < 2$ to 5.

The *key* point is that the CBL turbulence structure should be characterized by $-z_i/L$ or any of the other dimensionless quantities (w_* /U or w_* /u_*) instead of H_0 (pages 2-10, 2-14). On page 2-15, it is stated that when $U > 6w_*$, convective scaling is no longer assumed and the PBL is assumed to be in a neutral state. This characterization (U/w_*) should replace H_0 as the main stability variable for unstable conditions on pages 2-10, 2-14, and elsewhere.

Holtslag and Nieuwstadt (1986) discussed a similar turbulence characterization for the SBL using the ratio h/L, where h is the SBL depth. Again, a dimensionless variable rather than the dimensional H_0 should be used to determine the stability of the SBL (pages 2-10, 2-16, and elsewhere).

2. Special treatment for highly buoyant plumes in the CBL, $F_* > 0.1$

Laboratory experiments (Willis and Deardorff, 1983, 1987) and field observations (Weil et al., 1986; Hanna and Paine, 1989) show that highly buoyant plumes in the CBL--those with $F_* = F/w_*^2 Uz_i > 0.1$--can "loft" or remain near the top of the CBL and disperse more slowly than plumes with $F_* \sim 0.01$. The buoyancy-induced velocities within the plume are able to withstand the negative velocities due to the ambient convection until the local buoyancy ($\Delta\rho$) is decreased due to: 1) mixing and entrainment, and/or 2) the increase in the mean mixed layer temperature with time.

The highly buoyant plume problem requires a special model, currently not included in CAP, and failure to do so may result in significant overestimates of GLCs as discussed in Section II.1--predictions a factor of 2 to 3 greater than observations (see Weil, 1985, 1988b; Weil and Corio, 1988). The high F_* conditions typically occur with light winds ($U < 2$ m/s) and low z_i values ($z_i < 700$ m).

There have been some empirical models proposed for this situation (Briggs, 1985; Hanna and Paine, 1987, 1989) as well as some based on the physics of the problem. In the latter category, Weil and Corio (1988) considered the dispersion following a decrease in the plume buoyancy ($\Delta\rho$) due to mixing and entrainment, and Weil et al. (1991) addressed the dispersion resulting from a $\Delta\rho$ decrease due to the increase in the mean mixed layer temperature with time. The Weil and Corio model can be recommended as an interim approach for the high F_* case until a better one is available. I believe that the Weil et al. (1991) model ultimately will be a better approach, but it needs further development before it can be routinely applied.

3. Dispersion in "calm" conditions

In the CAP model, the treatment of dispersion in calm periods is the same as used in most other simple regulatory models--an arbitrarily small wind speed is assumed ($1.2w_*$ or 1.5 m/s). However, this treatment is unnecessary since a simple Gaussian puff model is available and should be applicable for convective periods with $F_* < 0.1$; the puff model includes along-wind or axial dispersion. The original model developed by Frenkiel (1953) applies to dispersion in an unbounded atmosphere--an infinite ratio, z_i/z_e. It incorporates the short-range limit of statistical theory for estimating σ_y and σ_z : $\sigma_y = \sigma_v\, x/U$, $\sigma_z = \sigma_w x/U$, where σ_v and σ_w are the rms turbulence components in the y and z directions and x is the downwind distance. The expression for the GLC, C(x,y,0), is

$$C(x,y,0) = \frac{Q\sigma_u \exp\left(\frac{-y^2}{2\sigma_v^2}\right)}{(2\pi)^{3/2}\sigma_v\sigma_w r^2}\left[1+\left(\frac{\pi}{2}\right)^{1/2}\frac{ux}{\sigma_u r}\exp\left(\frac{u^2x^2}{2\sigma_u^2 r^2}\right)\operatorname{erfc}\left(-\frac{ux}{\sqrt{2}\,\sigma_u r}\right)\right] \qquad (1)$$

where y is the lateral distance from the mean plume axis, σ_u is the longitudinal turbulence component, Q is the emission rate and r is

$$r = \left(x^2 + \frac{\sigma_u^2}{\sigma_v^2}y^2 + \frac{\sigma_u^2}{\sigma_w^2}z_e^2\right)^{1/2}. \qquad (2)$$

A key advantage of (1) is that it is valid for an arbitrary wind speed including $U = 0$.

Weil and Brower (1985) adapted this model to a finite mixing region (i.e., the CBL) and included plume reflection at z_i as well as at $z = 0$. Exploratory runs were made for $z_e/z_i = 0.8$, and the GLC was compared to that from a "continuous" (Gaussian) plume model, i.e., with along-wind diffusion neglected. The comparisons showed that the two models gave comparable results on the maximum GLCs for $U/w_* > 1.5$, but for $U/w_* < 1.5$, the plume model overestimated the puff results. For example, the maximum GLC for the puff model was 16%, 40%, and 50% less than the plume model values for $U/w_* = 1.1$, 0.8, and 0.5, respectively; see Fig. 1. The key point is that the effect of very light and zero winds on the GLC can be incorporated through a puff model; this approach however, is probably limited to buoyant plumes with $F_* < 0.1$.

4. Dispersion in building wakes (Appendix A)

The main limitation in the wake dispersion model is the neglect of source momentum and buoyancy in the concentration estimates. This problem needs to be

addressed from a combined analytical and physical modeling approach. In addition to this, there are a few other limitations or questions about the downwash model.

a) Equation (A.7.1) is dimensionally inconsistent and should not be used since the wind speed is omitted. The equation following (A.7.1), $C = Q/(a_1 U A_p)$, is the preferred form. This is consistent with the results of Huber and Snyder (1976) who found that with $A_p = H_B^2$ and $U = U_B$, $a_1 = 1.7 - 2$; however, for an elevated source, the a_1 is larger than 2. Huber and Snyder found that for an elevated release, a different expression with a distance dependent σ_z based on a virtual source performed better. As noted earlier, I think that their dispersion estimates/results should be incorporated into CAP since they have a solid basis--wind tunnel experiments.

b) The approach for computing the concentration beyond the cavity region, i.e., with $z_e = 0$, is overlyconservative for sources of arbitrary height, $z_e/H_B > 1$. Figure 6 of Huber and Snyder (1976) shows that the axial GLC distribution downwind of a building varies significantly with the ratio z_e/H_B (i.e., the peak GLC decreases with increasing z_e). Only for $x/H_B > 30$ do the axial GLCs for the various source heights converge to the value for a near surface release.

5. Dispersion in the lakeshore environment (Appendix C)

There are four aspects of the lakeshore problem that should be reconsidered in my opinion. The first is the no penetration assumption for a trapped plume in the TIBL. This was discussed in Section II.4 and a recommendation was given there. I strongly urge the authors to reexamine this problem since with low TIBLs and light winds very large GLCs may result; this would be a high F_* problem.

The second concerns the TIBL model (for $z_i(x)$) and in particular the parameter A. The current formulation for A is based essentially on an assumed land temperature, θ_L, that is independent of fetch (x). However, I believe that a better assumption is that H_0 is independent of x as used in Weisman's (1976) model for example. Both assumptions give the same functional dependence of z_i on x., and in the constant H_0 case θ_L varies with x as actually occurs near lakeshores. In the heat flux formulation, A is

$$A = \left(\frac{2 H_0}{\rho c_p \partial \theta_w / \partial z \, U} \right)^{1/2}.$$

One practical advantage of this formulation is that all of the key variables enter to the 1/2 power; this would tend to minimize the effect of uncertainties in them on z_i. In the CAP formulation (Eq. C.5.3), A depends linearly on u_*/u_m.

A third aspect concerns the linear temperature profile in the TIBL model and therefore the appearance of $\partial\theta_w/\partial z$ in A. In many cases, the temperature profile may have an arbitrary and nonlinear shape. Weil and Brower (1983) considered the

development of z_i over land for such a profile shape and adapted Carson's (1973) model. This would be simple to implement for the TIBL since the energy balance or slab model for z_i is essentially the same for H.

The fourth aspect is the coefficient (1/3) in the σ_w model for the mixed layer. This coefficient, based on large-eddy simulations, underestimates the lateral dispersion observed in the CBL (Weil, 1988b). For σ_v, I would recommend the CAP Eq. (4.14) instead of C.5.6.

IV. ITEMIZED COMMENTS ON THE CAP MODEL

1. page 2-5. It is stated that when the wind speed exceeds 6 m/s the mechanical turbulence may dominate the PBL turbulence; this depends on the magnitude of H_0, z_0, and z_i as discussed earlier. Here and elsewhere, the height of measurement should be stated. It might be best to just eliminate the mentioning of a specific wind speed of 6 m/s here.

2. page 2-15. It seems unnecessary to use a mean CBL temperature, $\bar{T}$, since the difference between $\bar{T}$ and T_a is small, especially in ^0K, for a nearly well-mixed PBL. Here, it should be emphasized that the temperatures are absolute, i.e., in ^0K.

3. page 2-15. The discussion about the consistency or lack of it between the z_i determined: 1) from the morning sounding and surface temperature, and 2) the z_i indicated by the value of L is totally confused. z_i is an independent parameter determined from 1) above and L is determined from the independent parameters u_* and H_0. I do not see how L can infer a value of z_i. Thus, the discussion about changing z_i based on the L value is meaningless. I think z_i should be based on 1) above (surface temperature extrapolation) or a simple energy balance model (see Weil, 1988b). L should not enter into this.

4. page 2-16. There is really little validation for the NBL height (H_N) formula, Eq. (2.2.8). For daytime periods, I think an estimate of z_i should be made and then the smaller of z_i and H_N should be used for the PBL height H.

5. pages 2-19,21. Here and elsewhere, it should be made clear that they are assuming the initial vertical dispersion $\sigma_{z0} = 0.5H_A$ or $0.5H_B$.

6. pages 2-22,23. The method for determining S throughout the SBL may lead to poor estimates of it and $\partial\theta/\partial z$ because surface layer similarity does not hold throughout the SBL. The author's should consult the models of Stull (1983) and

Wetzel (1982) for estimating $\partial\theta/\partial z$ in the SBL, and the discussion by van Ulden and Holtslag (1985).

7. page 2-24. The coefficient 3 in Eq. (3.2.8) was 4.3 in the earlier model by Briggs (1975). Weil (1979) found 4.3 to be more consistent with plume rise observations at the Morgantown power plant than the coefficient (3) given by Briggs (1984). Weil and Brower (1984) used the original coefficient (4.3) in their PPSP model.

8. page 2-24. The authors should note that the final rise formula (Eq. 3.2.10) must be solved iteratively for Δh_{ot}. Briggs recommended a simpler analytical expression that approximates this expression, i.e., it avoids iteration; see Eq. (4) in Weil (1985).

9. page 2-28. The inversion jump $\Delta\theta$ = 10°C seems much too high for typical inversions capping the CBL. A value like 1 or 2°C would seem more appropriate as mentioned earlier. Also, the authors should consider a penetration model based on a thick stable layer capping the CBL (Weil, 1988a) instead of the "jump" model.

10. page 2-29. Where does Eq. (3.4.6) come from? For plume material trapped in the mixed layer, the limiting z_e should be H. However, there could be situations where (3.4.6) gives $z_e > H$. The authors may wish to consult the z_e model given by Weil and Brower (1984) since it satisfies the limiting z_e (= H).

11. page 2-30. The Nieuwstadt (1980) model is only applicable to a surface release because it is based on the free convection result $\sigma_z \propto x^{3/2}$ which only holds for $z_s < 0.001 z_i$ (see Weil, 1990). I think that Deardorff and Willis' (1975) interpolation expression for σ_z would be better since it holds for both surface sources and elevated sources within the convective surface layer. Either of these models should be used with the Gaussian plume model to estimate the concentration field; Weil (1988b) discusses the use/applicability of the Deardorff and Willis σ_z result. It should be used up to $0.1 z_i$ with the Venkatram (1980) model being used above that height.

12. page 2-35. I don't know where the result $\sigma_z = H/(2\pi)^{1/2}$ comes from. For a neutrally buoyant plume and a well-mixed vertical distribution far downstream the σ_z is given by

$$\frac{\sigma_z}{z_i} = \left[\frac{1}{3} - \frac{z_s}{4 z_i} + \left(\frac{z_s}{2 z_i}\right)^2 \right]^{1/2}$$

(see Eq. 4.36 of Weil, 1988b).

13. page 2-35. The above expressions for σ_z should not be used in the Gaussian plume model (Eq. 4.2.11) to estimate GLCs; that approach would probably tend to underestimate the GLCs far downstream because of the neglect of image sources in Eq. (4.2.11) to satisfy the no-flux boundary condition at $z = z_i$. The distance dependent σ_z should be used in the Gaussian model that includes image sources. Once the plume becomes uniformly mixed in the vertical, the simpler expression for the GLC can be used:

$$C(x,y) = \frac{Q}{\sqrt{2\pi}\,\sigma_y U z_i} \exp\left(-\frac{y^2}{2\sigma_y^2}\right).$$

14. page 2-36. The surface similarity model used for surface releases in neutral and stable conditions is limited to modest downwind distances such that the plume has not spread too far into the NBL or SBL where the similarity expressions do not hold; my guess is that this approach only holds for $\bar{z} < (0.1\text{-}0.2)\,H$. For guidance on this limitation, see Gryning et al. (1987).

15. page 2-38. The Venkatram et al. (1984) σ_z formulation would be better for elevated releases in the SBL than the approach on page 2-38. The Venkatram et al. model has been tested against limited field observations.

16. page 2-61. Do TIBLs never develop during the winter months? Some reference or discussion should be given for the criterion of $\theta_L - \theta_w > 3^0C$.

V. RECOMMENDATIONS ON IMPLEMENTING CAP

I have reviewed and discussed many aspects of the CAP model and indicated a number of problems that need to be reconsidered. Of the main limitations (Section III), I believe that all except perhaps for the "calm" model should be addressed before CAP is implemented. In addition, I highly recommend a reassessment of the data requirements (Section II.3) and conservative assumptions (Section II.4). Once the technical modifications have been made to CAP, I strongly urge that it be evaluated for predictions of maximum GLCs around existing sources using year-long data records. That is, test CAP in a mode that it will be used in. This test should be on magnitudes and frequency of occurrence of high GLCs occurring over a year to ensure that predictions and observations are in "ball park" agreement. Surprises or poor performance would be best known at this time rather than after the model is fully implemented for regulatory purposes.

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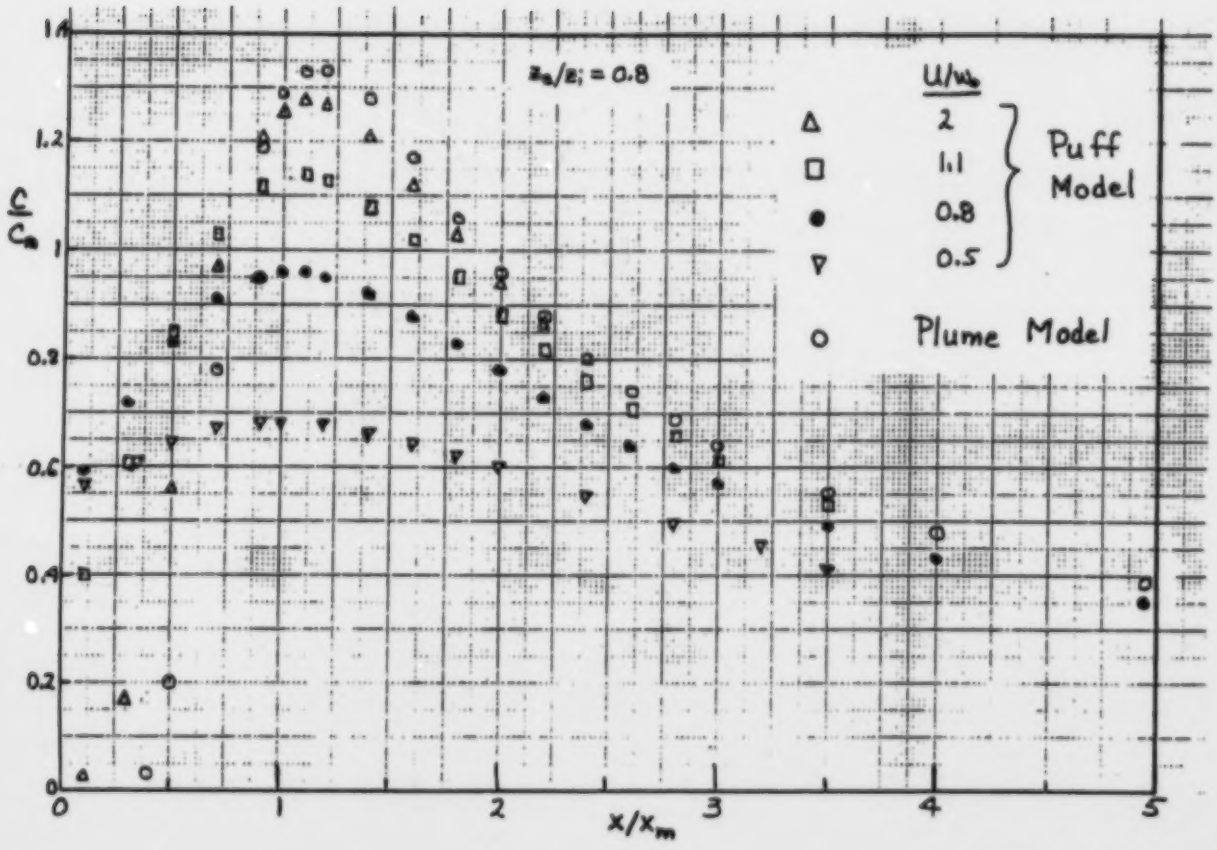

Figure 1. The predicted ground-level concentration (GLC) along the plume centerline (y = 0) as a function of the downwind distance, x, in the convective boundary layer for Gaussian plume and puff models. The concentration is nondimensionalized by the maximum GLC (C_m) given by the Gaussian plume model for an infinitely deep mixed layer ($z_i \rightarrow \infty$) and x is nondimensionalized by the distance (x_m) to that maximum. The results here are for $z_e/z_i = 0.8$. The maximum GLC for the plume and some of the puff model results exceeds C_m due to the effect of plume trapping by the elevated inversion.

APPENDIX C

Steven R. Hanna

Review of Pages 2-1 through 2-68 of Appendix 8 (Air Quality Modelling)
of the Ontario Ministry of the Environment (OME)
Clean Air Program (CAP) Modelling Package

by

Steven R. Hanna
December 23, 1990

1. Introduction

Because the Ontario Hydro facilities are strongly affected by the air quality models discussed in the OME Air Quality Modelling report, Ontario Hydro is concerned that these models are based on sound scientific principles. The emphasis of the OME report is on short range models, such as Gaussian plume models, that are applicable for averaging times of 30 minutes or greater, and are intended for sources in which nonreactive, non-dense contaminants are released at a relatively continuous rate. These models account for the influence of shorelines, building obstructions, and other local effects. The models were selected following a comprehensive review of the literature.

2. General Comments

The following general comments apply to the entire OME report:

2.1) <u>The scientific algorithms are 5-10 years out of date</u>.
It is clear from the text and from the list of references that most of the OME scientific review was conducted in 1983. I appreciate the fact that it takes a long time to obtain approvals for these reports and procedures within OME, but wish to point out that much work in these areas has taken place in the past seven years. A good starting list of recent references would include Irwin and Paumier (1990), Hanna and Paine (1989), Weil and Brower (1984), Weil and Corio (1988), Oleson (1988), and Van Ulden and Holtslag (1985).

2.2) <u>There are many unjustified empirical constants</u>.
The report briefly summarizes a large number of models and equations for specific air quality scenarios. Unjustified empirical constants and assumptions appear in many of these models. There are no references or figures demonstrating the origins of the constants. I am not sure whether detailed back-up information is needed in a summary report such as this, or whether there should be separate reports providing these analyses for each component.

On the other hand, the OME is to be commended for providing as much scientific justifications as they do. The USEPA UNAMAP models are even more scimpy in their explanations and did not invite comments from industries at this stage.

2.3) There is no evidence that the models have been adequately tested.
At the time that each algorithm is discussed, there is some discussion of previous testing as reported in the original reference. In addition, if I read between the lines, there are suggestions of model testing by OME. However, this report contains no plots of model predictions versus observations, and consequently no statistical analyses.

2.4) The degree of conservatism is unclear.
The emphasis of the OME report seems to be on scientific credibility across the entire range of the concentration distribution. I see no indications that conservative factors have been introduced, although I admit that this question is seldom discussed. We would be better able to answer this question if back-up tables and plots had been provided for each assumption.

2.5) The building wake algorithm is not widely supported.
The OME's building wake model departs from the USEPA's model at several points, and no justification is given for the procedures and constants that are assumed.

2.6) The data requirements are realistic.
The OME has suggested a set of realistic data requirements that represent a good balance between practicality and scientific needs.

3. Specific Comments

The following specific comments are given in chronological order.

p2-1: It should be mentioned whether the emphasis is on the extreme high end of the concentration distribution function, as is true of the U.S. EPA procedures.

p2-1. last paragraph: Positive surface heat fluxes are also associated with cloudy daytime conditions with high winds. In these conditions large convective eddies do not form.

p2-2 and 2-3, Table 1.1: This two part table could be made much clearer. It looks like it was originally made on a typewriter and enlarged on a copy machine. It should be redrawn by a graphics department. In part a, add the word "sources" to the columns labeled "near surface" and "elevated".

p2-4, Figure 1.1: Stress that this figure is valid only for clear skies. Why is there a double line at about 8-9 am. and a "bounce" at about 8 pm?

p2-5, line 2: The phenomena that are being discussed are valid for clear-sky conditions. Boundary layer heights can be less than 50m at those times. However, it is possible to have a negative heat flux associated with cloudy skies with high winds at night, when boundary layer heights can be 500m or more.

p2-5, line 5: Define what is meant by a surface heat flux that is "essentially zero".

p2-5, par 4: At what height should the wind speed be observed in order to trigger the neutral stability classification?

p2-6, line above 1.3.2: This procedure is usually called the "virtual source" method.

p2-6, line 3 from bottom: The 15km criterion for considering shoreline effects should be better justified. The same comment applies to the criteria for complex terrain discussed in Section 1.3.2.2 on pages 2-7.

p2-7, last paragraph: The half-height assumption is usually assumed only for neutral and unstable conditions in models for flow over complex terrain. For stable conditions, there is typically assumed to be no plume lifting.

p2-9, line 6 from bottom: A reference is needed for the roughness length assumption. I have seen a 1/10 ratio used more often than a 1/30 ratio.

p2-10, Table 2.1: The heat flux criterion for neutral conditions seems rather restrictive. The stability also depends on the wind shear (a principle embodied in the Richardson number). The boundary layer height is important for neutral conditions, too.

p2-12: The effects of latent heat seem to be parameterized by a single constant. Other recent meteorological preprocessors (Van Ulden and Holtslag 1985, Oleson 1988, and Hanna and Paine 1989) explicitly account for the latent heat flux. The larger the latent heat flux, the smaller the sensible heat flux, thus leading to reduced turbulence and diffusion.

p2-14, Table 2.4: Why isn't the boundary layer height required for convective conditions? It was discussed on the previous page and is used in Equation (2.2.1).

p2-15, top line: Stress that T is in units of Kelvin.

p2-15, line 7: The Van Karman constant is fairly important in these equations, since several parameters such as L and u_* are linearly related to this constant. Most applied models use 0.40. Justification should be given for the 0.35 value.

p2-15, middle paragraph: These relations between z_i and L seem reasonable, but I have never seen them discussed in the literature. Is there a reference?

p2-15, last paragraph: The same comment applies to the mixing depth and the minimum wind speed assumptions.

p2-18, bottom: The section on influences of structure wakes contains formulas that are similar to those in the USEPA's ISC model, but the constants are different. The predictions of the two models should be compared with each other and with field and laboratory data.

p2-19, eq. (3.1.2): This symbol was used earlier for the roughness length.

p2-20, eq. (3.1.4) and p2-21, eq. (3.1.5): The standard deviation of a uniform distribution of width 2R is $R/\sqrt{2}$, not R/2.

p2-22, eqs. (3.2.1) and (3.2.2): It should be mentioned that pi has been removed from the flux definitions.

p2-23, eq. (3.2.4): This equation for S is a good idea but why is 2L chosen for a height limit? Furthermore, I believe that the temperature gradient at stack height may be calculated to be unrealistically large at times. This would lead to underpredictions of plume rise. Has this procedure been tested with a long time period of data?

p2-24, eq. (3.2.8): This equation is valid only for $F_m < 1$.

p2-25, eq. (3.2.11): It is implied that this equation is best used for neutral conditions. Actually, it is valid near the stack for all ambient stabilities, including convective and stable conditions. This comment also applies to line 2 above Section 3.2.4, where the word "neutral" should be crossed out.

p2-25, section 3.2.4: This is a good explanation of the effects of moist plumes.

p2-26, bottom: The constant, 4, should be replaced by $2\sqrt{2}$.

p2-27: Section 3.3.1 is too brief. More explanations of the assumptions in the complex terrain algorithm are needed. Under stable conditions, most scientists assume no plume lifting over terrain.

p2-28, middle paragraph: During the past five years, Weil and his colleagues (Weil and Brower 1984, Weil and Corio 1988) have developed an analytical model for calculating plume lofting and penetration into elevated capping inversions. These algorithms have been incorporated into the latest version of the HPDM model. The authors should review this research.

p2-29: Section 3.4.3 contains only two sentences, and is not understandable.

p2-30: It is a good idea to separate the procedures used for near surface releases from those used for elevated releases. Was any attempt made to insure a smooth transition between the two regimes?

p2-31, eq. (4.1.5): In the USEPA ISC model, the quadratic sum is not used. Instead, there is a transition from wake-induced spread to the turbulent sigma-y at some distance downwind.

p2-32, top 3 lines: This virtual source assumption seems rather arbitrary. Is there a published reference for this procedure?

p2-32, section 4.1.2: If this model is based on Venkatram's (1980a) paper, this fact should be mentioned at the beginning of the section. It would help if this discussion were expanded. Was Weil and Brower's (1984) pdf model considered?

p2-33. line 3 of section 4.2: Define "greater". Does z = 0.1m qualify?

p2-33: Equation (4.2.1) is a repeat of equation (4.1.2).

p2-34, eq. (4.2.4a): I thought that $\sigma_v \approx 2u_*$. In this equation, it looks like $\sigma_v = 1.3u_*$. Also, the denominator in equations (4.2.4a) and (4.2.4b) was not suggested by Wyngaard et al (1974). Please provide a reference.

p2-34, line above eq. (4.2.5): Poor grammar.

p2-34: Justify the maximum limit to σ_z.

p2-36: Provide references for the assumptions regarding "near-surface", "maximum value of z", and the function suggested in Equation (4.3.6).

p2-36: Equation (4.3.1) is a repeat of Equations (4.2.1) and (4.1.2).

p2-37 and 2-38: What should be done if the release height is greater than the mixing depth? This situation will occur often for Ontario Hydro stacks. For example, during cloudy and windy nighttime conditions, the mixing depth could be 100m and plume height could be 150m. In this case the plume would be likely to be mixed to the ground at a downwind distance of about 2km. Yet it appears that the OME model would assume zero turbulent dispersion in this situation.

p2-47, line 5: Usually the minimum L is defined as the lesser of the building height and the width.

p2-47, line 6: What is the reference for the height criterion? It would help if a diagram showing these definitions were provided.

p2-47, line above A.3: The physical modeling should be done under the advice of the agencies, as well as "at the discretion of the proponent."

p2-47, last paragraph: This is different from USEPA procedures, which state that if the separation distance is less than L_{min}, then the buildings are treated as one, using the height of the shorter building to define L_{min}.

p2-48, line 4 after A.4.1: Again, there are slight discrepancies in the constants used in the USEPA and OME models. The USEPA uses $1.5 H_g$ as a criterion.

p2-49, line after A.6: The degree of trapping depends on the height of release. How is this fact taken into account?

p2-49, line 9 from bottom: But USEPA (ie, Huber) says $2H_g$ upwind or $5H_g$ downwind.

p2-50, line after A.7.1: Define "near".

p2-50, eq. (A.7.1): Wilson recommends $C = 1.6Q/ux^2$. Are these equations similar? The many empirical assumptions on this page should be justified by references or by comparisons with data. The model should be compared with EPA and NRC models.

p2-51, eq. (A.7.2): This is a very conservative assumption. Other models include the effects of non-zero release heights.

p2-51, bottom: Huber states that σ_z is not enhanced if the stack height is greater than $1.2\,H_s$.

p2-54: Give the units for B and H. Incidentally, the symbol H was used for another variable earlier in this report.

pp2-51 and 2-57: This general discussion is good. Most references do not provide these detailed explanations.

p2-57, line after C.3: Justify the 15km assumption.
last line: Are the units correct in this definition?

p2-58, top line: Please justify the 50m and 100m assumptions for the shoreline boundary layer height.

p2-59: What is the accuracy claimed for the shoreline fumigation model? How does it compare with the EPA's shoreline dispersion model, which is based on Sethuraman's research?

p2-60, eq. (C.5.3): Our tests with this formula suggest that it is too sensitive to the observed land and water temperatures and vertical temperature gradient. These parameters are not well-defined. For example, at what height is the vertical temperature gradient observed?

p2-60, eq. (C.5.4c): Where did the constant, 3.7, come from?

p2-61, line 9: It is stated that "the model predictions agree well with the observed values." Please provide quantitative back-up for this statement. For example, what is the root-mean-square-error?

p2-61, bottom: This procedure can lead to spurious temperature differences. The USDOI's OCD model tested the same method but does not use it any more. If no information is available, then a simple climatological assumption should be made for .

p2-62, line 9: Provide a reference for this default value of the temperature gradient.

pp2-64 and 65: What papers or reports can be cited that justify these arbitrary assumptions?

p2-65, middle: It is unreasonable to require that a facility be capable of simulating all stability conditions. This cuts out most of the commercial wind tunnels.

p2-66, last paragraph: The half-height assumption is not conservative for stable conditions.

Appendix B-3: Most of the material in this discussion of the meteorological parameters is similar to the material in the section reviewed above. These same review comments apply to Appendix B-3. There are many arbitrary assumptions (e.g. for defining shoreline, urban, and hill or valley environments) that should be supported by references.

References:

Baerentsen, J.H. and R. Berkowicz, 1984: Monte Carlo simulation of plume dispersion in the convective boundary layer. Atmos. Environ., 18, 701-712.

Hanna, S.R. and R.J. Paine, 1989: Hybrid Plume Dispersion Model (HPDM) development and evaluation. J. Appl. Meteorol., 28, 206-224.

Irwin, J.S. and J.O. Paumier, 1990: Characterizing the dispersive state of convestive boundary layers for applied dispersion modeling. Bound. Lay. Meteorol., 53, 267-296.

Oleson, H.R., 1988: Users Guide for CML-POINT, An Air Pollution Model for Point Sources. MST LUFT-A1255, National Agency of Environmental Protection, Roskilde, Denmark.

Van Ulden, A.P. and A.A.M. Holtslag, 1985: Estimation of atmospheric boundary layer parameters for diffusion applications. J. Clim. and Appl. Meteorol., 24, 1196-1207.

Weil, J.C. and R.C. Brower, 1984: An updated Gaussian plume model for tall stacks. J. Air Poll. Control Assoc., 34, 818-827.

Weil, J.C. and L.A. Corio, 1988: A modification of the PPSP dispersion model for highly buoyant plumes. Maryland Power Plant Research Program. Maryland Department of Natural Resources. Annapolis, MD. Report No. PPSP-MP-50.